say I am aware of anything of the kind. The "late Prof. Nägeli" may be dead against me possibly; but I was not aware that he was dead in any other sense. Nor do I see that as he insists (quite correctly as I think) on the inutility of family characters he can afford much comfort to Mr. Romanes, who regards them as generally adaptive.

I have devoted a good deal of time to the study of Mr. Romanes's paper published by the Linnean Society. I believe I have stated the conclusions to be drawn from that paper with tolerable accuracy. If I have not done so, I undoubtedly owe him a sincere apology. But I am bound to confess that, the more I study his views, the more I find myself in disagreement with him as to the inutility of specific characters; as to the utility and mode of origin of generic characters and those of higher grade; as to sterility as a primary specific difference; and as to the value of so-called physiological selection. In all these matters he is, I am satisfied, contradicted by botanical experience. I think if he had imitated the example of Mr. Darwin, and had carefully collected a large body of evidence on each of these points with a perfectly open mind, he would have found this out for himself. What, however, I view with less patience than his unsustained generalizations, is his persistent attempt to place them on the shoulders of the Darwinian theory. I have reluctantly arrived at the conviction that his only excuse for so doing is that he has fundamentally misunderstood that theory. At any rate, I cannot in any other way account for the strained interpretation which he has put on passages from Mr. Darwin's writings. I may give, as an example, the passage he quotes "to justify the insinuation" that the "Origin of Species" has been misnamed; the obvious drift of this does not relate to specific differences at all, but to those which are characteristic of families. It is easy to see, in fact, by a comparison of pp. 170 and 176 of the sixth edition, that the passage cited by Mr. Romanes was inserted by Mr. Darwin to meet the point raised by Nägeli to which I have referred above. Certainly I think that no one would have been more surprised than Mr. Darwin when he wrote the words could he have foreseen that they would be used to impugn the validity of the title of his theory and of his book. Everyone knows that Mr. Darwin was the fairest and most generous-minded of men. He constantly admits the possibility of explanations to which he really, however, did not attach much importance. Such admissions Mr. Romanes appears to me to treat as if wrung from a hostile witness. In my judgment this is entirely to misapprehend their significance or the spirit in which they were made. W. T. THISELTON-DYER.

Royal Gardens, Kew, December 1.

Natural Selection and Useless Structures.

IN his letter on "Mr. Romanes's Paradox" (NATURE, November 1, p. 7), Mr. Thiselton Dyer questions the existence of indifferent or slightly disadvantageous specific characters. That letter referred, in a highly laudatory yet somewhat deprecating manner, to a lately published (Proc. Roy. Soc., No. 269) obituary notice of Mr. Darwin; and it implied that Mr. G. J. Romanes, from his unfamiliarity with the study of species, did not quite know what he was talking about when he asserted that such indifferent characters do in fact exist. I, who claim to have had some slight experience in the practical discrimination of species, ask permission to make a few observations in your columns on the subject.

Everyone would, I suppose, regard the frequent absence of the toe-nail on the hallux of the orang as an indifferent matter, but I am inclined to consider the feeble development of that digit itself as a slightly disadvantageous one. However that may be, I am strongly of opinion that the abortion of the index in the Potto can never have saved the lives of the earliest individuals so distinguished. I have, as yet, heard no reason assigned for the life-saving action of the thumbless hands of *Colobus* and *Ateles*, or of the tail of the one chameleon in which alone (so far as I know) that organ is not prehensile. The metallic lustre of the peritoneum of some fishes is hard to explain by either "natural" or "sexual" selection; as also are such specific characters as the extension, or non-extension, of the premaxillæ to the frontals, or the pattern of the foldings of enamel and cement in various Rodents. The complexity of the teeth of *Labyrinthodon*, or the similar multiplicity co-existing in those of *Orycteropus* and *Myliobatis* (which can hardly have been derived from a common ancestor, though their resemblance extends even to microscopic structure), are unquestionably good taxidermic characters; yet they can hardly have been due to the action of natural selection, as I pointed out in my "Genesis of Species" in 1870. But if such "selection" cannot originate characters which form the diagnosis of a species, then it cannot possibly be the origin of such species. To say that the rudimentary index of the Potto is a character which, though itself useless, has been carried on the back, as it were, of some possible but unknown useful simultaneous variation which co exists with it or did co-exist with it in some unknown ancestor is a purely gratuitous assertion. Such assertions are the less warranted because we have evidence that the energy of Nature's destructive forces has been exaggerated. Prof. Dyer tells us that natural selection is a hard taskmaster; but it is not, I think, so hard a one as some persons suppose. This seems to me clear from such facts as the finding of hares and rabbits in which an incisor tooth has grown so as to complete the circle it always tends to form—a condition which shows a remarkable preservation of life under extremely disadvantageous circumstances. A stoat, three of whose feet had been cut off at different times by traps, has nevertheless (I am informed) lived long enough for its injured limbs to heal so thoroughly that the beast could get a living on its one foot and three stumps. Cases of prolonged life under trying circumstances are not so rare. I recollect the skeleton of a monkey which must have long suffered from acute rheumatism in its native forests.

Prof. Dyer deprecates the admission, by the author of the obituary notice, that indifferent or slightly disadvantageous characters may be evolved in spite of "natural selection." But the obituary notice admits *much more than that*, since, according to its author, a maintainer of "natural selection" is free to affirm the genesis of species by sudden, considerable, definite variations, directly produced by the reaction of the innermost nature of an organism on the stimulus of its environment, according to precise innate laws of its being. This certainly is not "natural selection," as understood and taught by Mr. Darwin, and the inventor of a new term has alone the right to fix what its meaning shall be.

The statement of the obituary notice seems equivalent to an unintentional but virtual abandonment of "natural selection," while still retaining the name—reducing it, in effect, to that merely subordinate *rôle* we all admit that it plays. To call such a mode of origin "origin by natural selection" seems much the same thing as declaring an elaborately prepared theatrical transformation scene to be brought about by the chains and cords which prevent its moving pieces from passing beyond their assigned limits. The true meaning of "natural selection" is frankly declared by that distinguished biologist upon whose shoulders the mantle of the deceased prophet seems to have fallen. Prof. Lankester, in his article "Zoology" (in the last volume of the "Encyclopædia Britannica") has just given a most straightforward, lucid, and forcible representation of Darwinism. Nevertheless, the article (in the same volume) on "Variation" by Prof. Geddes, appears to me to be more in harmony with the facts of biology. It is, of course, open to anyone to say: "All species which succeed do so from some cause, and this may be metaphorically said to 'select' them." Therefore, since all causes are "natural" causes, every species which does succeed must succeed through "natural selection." This is equivalent to saying: "Nature is so conditioned as to produce the results it does produce"—an assertion most true, but somewhat trivial. When a term is so stretched as to mean "anything," it thereby comes to mean "nothing," and its use can serve no purpose save the preservation of a phrase it may be desired, for some reason, not to discard. ST. GEORGE MIVART.

Hurstcote, Chilworth, Surrey, November 28.

A Mussel living in the Branchiæ of a Crab.

LATE this autumn, while searching for Crustacea at Amroth, in South Wales, I found rather an exceptionally good specimen of the common shore crab (*Carcinus mænas*), which I took back to the hotel to clean and preserve. On removing the carapace, I found a mussel living among the branchiæ, and fastened to them by means of its byssus. It was in good condition, and measured ⅜ of an inch in length. The carapace of the crab measured 2¼ inches wide by 1⅞ inches long. I could find no signs on the exterior of the crab of anything remarkable within, nor was there any damage to the shell, or hole through which the mussel could have passed. It seems that the mussel, while yet minute, or in a larval condition, must have been carried

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into the branchiæ, along the ordinary passages, by the flow of water the crab urges through them; it must there have become entangled in the feathery branches, and lived in this unwonted habitat long enough to have grown to its present size, having its food carried to it by the same water that served to oxygenate the lungs of its host. W. R. PIDGEON.

42 Porchester Square, W.

The Pasteur Institute.

IN the article in NATURE under the above title the writer says (p. 74):—"The probability of rabies following thebite of a rabid dog is now definitely ascertained to be from 15 to 16 per cent. of those attacked." It would greatly assist all who desire to form an impartial estimate of the value of Pasteur's researches on rabies as far as they are deducible from a comparison of statistics, if the writer would state the facts and figures on which the above computation of 15 to 16 per cent. rests. The statement is repeatedly made, but the proof is never given along with it. It is obvious that, unless this percentage is proved beyond dispute, the statistical argument will be lacking in cogency and force, and leaves a loophole for attack by those who are ever ready to depreciate and oppose the brilliant investigations of M. Pasteur. ERNEST ALBERT PARKYEN.

Blackburn, December 3.

The Zodiacal Light.

IN your issue of October 25 (vol. xxxviii. p. 618), Dr. Muirhead quotes a remark of Cassini's in contradistinction to the relation indicated in your issue of the previous week (October 18, p. 594). The remark has not escaped notice, but is, I think, directed to a variation of shorter period, abundantly exemplified in Weber's observations, and in no wise invalidating the relation in the note of October 18. As far as Cassini's numerical observations go, the relation of the 18th is fairly exemplified, as will be shown by the following figures:—

Year.	Number of Observations.	Mean Elongation, referring to March.
1683	4	51·3
1684	2	67·10
1685 (max.)	33	52·35
1686	26	56·28
1687	16	68·82
1688	2	51·53

There are also observations indicating that the appearance did not pass away in 1688. Missionaries report brilliant appearances in 1690.

Any statement beyond the existence of this shorter variation would be at present premature.

The number of observations do not permit of a sufficiently sharp determination of the critical epoch to assert the amount of lag. The best determination which I am at present able to make is as follows. The sun-spot maxima of 1848, 1860, 1871, and 1883, follow the minima of the zodiacal light by + 1, − 1·5, − 1·5, + 1·5 years respectfully. The sun-spot minima of 1856, 1867, and 1878, follow the maxima of the zodiacal light by + 1, − 1, + 2·5 years respectively.

As to the working hypothesis, the suggestions put forward by Huggins in the Bakerian Lecture for 1885 seem in slightly varied form to meet all the facts which I am at present able to bring to bear upon the subject. There is evidence in the variation in the light of Encke's comet, as well as in the disturbance of its motion, that approaching the time of sun-spot maximum it meets matter moving towards the sun which it does not meet at the time of sun-spot minimum. Whence this matter comes may perhaps be questioned.

Observations seem to have been very nearly dropped since Weber's death in 1883. I am sure Dr. Muirhead will join with me in calling the attention of observers to this subject, and in asking that those observatories favourably situated would give us continuous records both as regards place, spectrum, and polarization. O. T. SHERMAN.

Baltimore, Md., November 15.

The "Tamarao" of the Philippine Islands.

DANS le numéro d'"August 16" (vol. xxxviii. p. 363), vous donnez une lettre du Dr. P. L. Sclater au sujet du *Tamarao* de Mindoro. Je crois que le Musée de Dresde s'en est déjà occupé; mais, sans avoir eu connaissance de ce travail, j'ai publié une note dans le tome ii. de nos Mémoires (Trübner, London) concernant l'histoire naturelle de l'Empire Chinois (p. 90), sur le *Tamarao*. J'y constate que c'est un bufle, et je propose de le nommer *Bubalus mindorensis*. Il n'a rien de commun avec l'*Anoa* des Célébes, au moins en ce qui concerne les dents.

Je suis curieux de voir dans le prochain numéro des P.Z S. une opinion contraire à celle de 1878. En dix ans on fait du chemin.

Je vous serais reconnaissant, Monsieur le Directeur, d'insérer ce petit mot dans votre correspondance. P.-M. HEUDE, S.J.

Musée de Zikawei, près Shanghai, 15 Octobre.

THE EARLIEST RACIAL PORTRAITS.

THE earliest representations of races that are preserved to us have been strangely neglected hitherto. On the Egyptian monuments are carefully sculptured and coloured figures of the various races that fell from time to time within the reach of conquest, or that entered into relations with Egypt, dating from the third millenium B.C.; yet till last year no attempt had been made to secure copies of these, free from the inevitable errors of mere drawings. At the desire of the British Association I took up this work, and made a series of casts of 280 heads from the sculptures, besides noting the colours of all paintings of races that I could find. These casts I then photographed, and the prints of the photographs can be obtained at cost price of printing.[1] These photographs are the source of the blocks (prepared by Messrs. Harper and Brothers) used in this paper, which, therefore, are perfectly automatic copies of the original sculptures.

In a recent article (NATURE, August 2, p. 321) Prof. Sayce has already noticed some of the conclusions to be drawn regarding a fair race in Palestine, so that it is needless here to repeat his statements; the actual portraits will, however, enforce his conclusions. The Amorites, who occupied the whole of Palestine, are seen (Fig. 1) to have fine though powerful features, quite different from the Jewish-Assyrian or the Egyptian types, with dolichocephalic heads; a type of face quite in accord with the light complexion and red-brown hair which they appear with in a painting of about 1500 B.C. They differ thoroughly from the features of the surrounding races of Hittites, Philistines, and Bedawin, as sculptured by the same artists, so that we are clear of the influence of mere conventionality. The Thahennu of Northern Africa, the Kabyles of modern times, show (Fig. 2) closely the same features, with only a slightly different beard and the long lock of side hair characteristic of the peoples of that region. Of the very few other portraits of Aryans that appear in Egypt, one of the most interesting (Fig. 3) is the primitive Greek woman, one of the captive Hanebu, or "lords of the north" (1400 B.C.). This has a very expressive and intelligent face, and the wavy sidelock and back hair recall the archaic Greek sculptures and vase-paintings. The stone has been unfortunately injured, but this precious proto-Greek is the only one remaining of the group.

In considering the origin of the Egyptians themselves, we are met with the difficulty that they are unlike any of the well-known neighbouring races. On the monuments we find, however, the Punites, or people of the southern shores of the Red Sea; and the resemblance between their features and those of the Egyptians is strikingly close. This noble of Pun (Fig. 4), has so precisely the face of Seti II, that either might be intended for the other. The evidence of relationship is not only in feature; the Egyptians coloured themselves as the red race, in contrast to the yellow Libyan, the brown Asiatic, and the black Negro in the four great divisions of mankind: they also colour the

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[1] Apply to Mr. Harman, 75 High Street, Bromley, Kent.

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